The Long Run Effects of Military Service: Evidence from the 911 Attacks

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Abstract

We investigate the effect of military service on labor market, health and family formation outcomes, leveraging differential changes in enlistment rates brought about by the September 11th attacks (911). Using restricted microdata, we identify hundreds of “high service" counties in which certain birth-county cohorts exhibit large enlistment responses to 911. We find that individuals born into high service counties between 1977 and 1983 (aged 18-24 at the time of the attack), enlisted at nearly twice the rate of earlier birth cohorts (older than 24 at the time of the attack). These high service birth-county cohorts experienced a 10% increase in wages, decreased unemployment and impacts on other labor market measures as well as key household formation measures including marriage and fertility. We also find increases in the hospitalization and mortality rates. Labor market benefits outweigh mortality costs at standard discount rates.

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1 Introduction

Approximately 20 million (15.4%) of the nation's households contain at least one veteran.[1] Thus, understanding the labor market and health impacts of military service are important not only for correctly assessing the well being of veterans, but also the well being of one in six US households. However, it is difficult to identify the impact of military service on an individual or a community because the decision to enlist is likely driven by many factors which are hard to measure directly in public datasets. These difficulties are perhaps especially true of the post-911 service era, which is characterized by voluntary, rather than conscriptive military service.

We investigate the effect of military service on labor market, health and family formation outcomes, leveraging differential changes in enlistment rates brought about by the September 11^{th} attacks (911). Using restricted microdata, we identify hundreds of "high service" counties in which certain birth-county cohorts exhibit sharp and persistent enlistments responses to 911. We find that individuals born into "high service" counties between 1977 and 1983 (who were aged 18-24 at the time of the attack), enlisted at nearly twice the rate of earlier birth cohorts (who were older than 24 at the time of the attack). This increased enlistment rates persists for at least a decade of birth cohorts after 911. These "high service" birth-county cohorts experienced a 10% increase in wages, decreased unemployment and impacts on other labor market measures. These birth-county cohorts also experienced increases in key household formation measures such as marriage and fertility. When we turn to health measures, we find that these birth-county cohorts also experience increases in hospitalization and mortality rates. Weighing the labor market benefits against the mortality costs, we show that, within standard ranges for the value of a statistical life and the intertemporal discount rate, the labor market benefits outweigh mortality costs.

[1] 2017 Census tabulations from SIPP data

The existing literature on the effects of military service has investigated pre-911 era enlistment, often relying on conscription rules, propensity score matching, or instrumental variables for identification (Angrist, 1990; Angrist and Krueger, 1994). Overall, the literature presents mixed and sometimes inconclusive evidence on labor market outcomes. Cesur et al. (2016) and Cesur et al. (2018) use post-911 era outcomes to study the effects of combat exposure (rather than the effects of enlistment more broadly) on mental health and criminality. Our work fills a gap in the literature by producing causal estimates of post-911 enlistment on labor market, family formation, and health outcomes. The effects of post-911 enlistment are directly relevant to emerging domestic and foreign policy considerations, as the War in Afghanistan ends.

2 Evidence on the Effects of Military Service and 911

2.1 Existing Evidence on Military Service Effects

Particularly in the United States, there has long been interest in the effects of military enlistment. Labor market considerations drive much interest with the armed forces being the largest employer of young people in the United States. In addition to the size of the military in the US labor force, there are also questions of equity because people of color disproportionately serve in the military (Dávila and Mora, 2012). The literature on military service while largely focused on labor market outcomes (the canonical example being Angrist and Krueger (1994)), also includes estimates on outcomes ranging from domestic violence to substance abuse (Cesur et al., 2016, 2018).

Existing research on the effects of military service on labor market outcomes have offered mixed and sometimes inconclusive evidence, especially by era of service (Angrist, 1990; Angrist and Krueger, 1994; Angrist et al., 1996). Research on the labor market effects of recent military service encounter an identification challenge due to self-selection and screen-

ing inherent in voluntary enlistment. For example, individual and family demographic and socioeconomic characteristics of veterans are different from civilians, and many of these characteristics are related to labor market and health outcomes (Dobkin and Shabani, 2009). Resultantly, much work has focused on variation resulting from conscription (i.e., "the draft"), which has been used intermittently in the US's history. Most of this economic research on the effects of military service focus on the period from 1940-1973 when the US military used both volunteers and conscription to fill the military. Because the military used both volunteers and conscription, however, papers on the effects of military service in the Vietnam and World War II eras, often rely on IVs to correct for selection bias (Angrist, 1990; Angrist and Krueger, 1994; Angrist et al., 1996).[2] Overall, these studies suggest show that being drafted hurts future earnings, in addition to the negative (mental and physical) health effects (Bedard and Deschênes, 2006; Hearst et al., 1986; Angrist et al., 2010).[3]

However, conscription ended in 1973 when the US moved to an all-volunteer military making this research less relevant to ongoing policy considerations, since military service during more recent wars may have different effects than service resulting from conscription. Estimates after the conscription era continued to rely on propensity score matching (PSM) and IV techniques to attempt to control for selection issues. For example, Angrist (1998) restricts their sample to applicants to the military in the early 1980s, matching for characteristics that the military used to screen applicants, and an IV based on an entrance exam scoring error that let previously unqualified individuals into the military. As Angrist (1998) emphasizes, these IV estimates are interpreted as local average treatment effects (LATE) that are tied to a specific intervention – in this case, the effect of military service on low-

[2]See also Imbens and Klaauw (1995) and Hjalmarsson and Lindquist (2019) for similar analyses of the effects of conscription in the Netherlands and Sweden.

[3]More specifically, studies examine effects of WWII, Korean War, and Vietnam War military service on mortality from smoking, motor vehicle deaths, and suicide (Bedard and Deschênes, 2006; Eisenberg and Rowe, 2009; Angrist et al., 1996). Other studies also use conscription IVs, but find limited evidence related to a range of health outcomes, such as alcohol consumption and AIDS (Dobkin and Shabani, 2009; Goldberg et al., 1991; Hearst et al., 1991).

scoring applicants – and not necessarily externally valid to military service more generally. Among these individuals, however, Angrist (1998) finds that military service in the early 1980s led to significant wage gains over comparable civilians while in the military and higher employment rates after service. Despite these higher employment for veterans, however, earnings were only slightly higher for veterans of color and lower for white veterans. These inconclusive estimates are identified using IV and PSM techniques.

Moreover, there is little well-identified research on the effects of post-911 military service in the era of the Iraq War and the War in Afghanistan, the longest war in the history of the US. Nonetheless, in addition to continued research with PSM, which provides some evidence of different effects than those found in studies on effects of military enlistment in earlier eras (e.g., Routon (2014)), much work exploits quasi-random variation in deployment to examine the effects of deployment on a variety of outcomes, with post-911 deployment is generally found to have large negative effects on physical health, mental health, and criminality (Cesur et al., 2013; Cesur and Sabia, 2016; Cesur et al., 2016). This work has not focused on labor market outcomes, and is limited to effects of *combat exposure*, rather than effect of *enlistment* more generally. Both effects are of interest, but for different reasons. As individuals have less control over combat exposure (which is the basis for their identification strategy), Cesur and colleague's results provide valuable information on the unique needs of post-combat veterans *relative to other veterans*. This article, on the other hand, examines effects of enlistment more generally (relative to non-veterans), an effect more relevant for discussing overall national policy related to veterans.

Existing research on the effects of military service on non-labor market outcomes also offer potential mechanisms for observed labor market effects. These mechanisms include educational attainment, family formation, and migration. The literature on these non-labor market outcomes often focuses on the effects of draft avoidance (college enrollment allowed draft avoidance during the Vietnam War) and GI-bills, which subsidized education costs.

Specifically, Card and Lemieux (2001) estimates that draft avoidance raised college attendance rates by 4-6 percentage points, while Angrist and Chen (2011), for example, find education gains of Vietnam-era military service can be attributed to veterans' use of the GI Bill rather than draft avoidance behavior. Research on the effects of the WWII-era GI bill suggests that there were substantial and positive effects on the educational attainment (Bound and Turner, 2002), though school segregation and discrimination in southeastern states led to benefits only accruing to White men (Turner and Bound, 2003). These historical evaluations, however, generally found modest economic returns to the schooling subsidized by the GI Bill (Angrist and Chen, 2011). Research on the effects of post-911 military enlistment on educational attainment (using state-level variation in education subsidization) indicates GI bill benefits increased college enrollment by about 20 percent (relative to other veterans) while also shifting the composition of enrollment toward four-year institutions (Barr, 2015).

Interacting with effects on educational attainment is the potential mechanism of migration. Other long-run consequences of conscription-era service include increases in migration (Angrist and Chen, 2011), with higher post-high school educational attainment significantly increasing the likelihood that individuals reside outside their birth states later in life (Malamud and Wozniak, 2012). In general, however, research has struggled to decompose migration as a potential mechanism due to data limitations.

Finally, another potential mechanism for labor market effects of military enlistment is household formation. Vietnam-era research (using conscription IVs) indicates military service reduces divorce rates for White men, and increases filial co-residence for men of other races (Heerwig and Conley, 2013). More recent research focuses on deployment effects, rather than enlistment more generally. For example, Angrist and Johnson (2000) examine the first Gulf War and find male soldier deployment has no effect on marital dissolution but does reduce spousal labor force participation, while deployment of women soldiers led to a large and statistically significant increase in divorce rates but does not reduce spousal labor force

participation. Post-911 research also focuses on the effects of deployment, finding significant increases in divorce rates, especially for deploying women and those who married before 911 (Negrusa et al., 2014).

In sum, well-identified effects of volunteer enlistment (and mechanisms for those effects) in general – and effects of post-911 enlistment in particular – are limited even though these effects are relevant to important policy considerations related to veterans of the longest war in US history.

2.2 911 as a Civil Service Shock

In response to the 911 attacks, US citizens engaged in a variety of service efforts including donating blood (Glynn et al., 2003), volunteering for local charities (Penner et al., 2005), and enlisting in the military. This increase in military enlistment is consistent with evidence that military enlistment is, at least in part, driven by engagement in civil service (Gorman and Thomas, 1991; Woodruff et al., 2006). Indeed, using restricted Census data that contains place of birth, we show how the increase in enlistment following 911 varies by birth-county birth-year cohorts. Figure 1 shows that together with a national downward trend in military enlistment across birth cohorts, for cohorts who were 18-24 during 911 the trend in military enlistment sharply increased relative to the trend in earlier born cohorts, and that this trend in enlistment began to fall again for cohorts who were younger than 18 at the time of the 911.

In the next section (Section 2.3), we show that this national trend break in enlistment was driven by enlistment increases in several hundred birth-counties, which we call "high service" counties. Our identification strategy relies on a difference-in-difference specification where we compare enlistment trends in these "high service" counties to all other counties, relying on the timing of births relative to 911 (Section 4).

2.3 Identifying High Service Counties

There are at least two accounts of why individuals enlist in the military post-911:

1. Military service is a form of civic engagement and civil service (Gorman and Thomas, 1991; Woodruff et al., 2006; Maley and Hawkins, 2018).

2. Military service is driven by a lack of economic opportunity (Kleykamp, 2006; Barr, 2016; Krebs and Ralston, 2020).

While public datasets show an increase in enlistments and the total active-duty military force following the 911 attacks,[4] these enlistment increases have not been broken down by place of birth. To identify the birth counties from which this post-911 enlistment increase originated, we take inspiration from accounts (1) and (2), and use Opportunity Atlas measures of economic opportunity and civic responsiveness at the birth-county level. The intuition behind our procedure is to use the best available measures of (1) and (2) to determine which birth counties would be most likely to encourage enlistment responses based on the economic opportunity and civic engagement of those counties. We call the birth counties most likely to yield an enlistment response "high service counties."

Our procedure for identifying "high service counties" is as follows:

1. Regress enlistment response against a quadratic polynomial in economic mobility, civic engagement and their interaction.

2. Based on the results of 1, isolate counties in specific deciles of the economic mobility and civic engagement variables for which we predict the largest enlistment responses to a shock such as 911.

3. Test whether the birth counties identified in 2 did in fact display a large enlistment response as predicted by 1.

[4] Author calculations based on public Defense Department reports "Demographics Profile of the Military Community".

The results of 1-3 are [Results awaiting Census Bureau disclosure.]

The final result of the procedure is the construction of an indicator variable which equals one if a birth-county is a "high service" county (based on that county's economic opportunity and civic engagement measures). Accordingly, we interpret the indicator variable for being a "high service" county as a cut on the upper most end of a summary index that tracks the civic and economic conditions predictive of an enlistment response to a shock like 911.

3 Data

3.1 Restricted Census Data

Our primary data source is an administratively linked longitudinal dataset constructed from linking the 2005-2018 American Community Surveys (ACS) to Social Security records (Numident).

The main benefits of linking the ACS surveys to Social Security records are that they allow us to:

1. Directly observe place of birth, and assign treatment based on place of birth (rather than place of residence at enlistment or ACS survey response)

2. Directly measure migration from place of birth to place of adulthood residence

3. Directly observe mortality using administrative, Social Security records.

(1) is crucial to how we assign treatment in this study (Section 4). We discuss the benefits of (2) in Section 6.3.1 and (3) in Section 6.2.

3.2 Opportunity Atlas: Economic Mobility and Civic Engagement

To identify high service counties we use Opportunity Atlas data. (The details of how we use these data are described in Section 2.3 above.) We utilize two county-level variables, one of which we take to be a measure of economic mobility in a birth county, and the other of which we take to be a proxy for civic engagement (or perhaps social capital) in a birth county. We note that Opportunity Atlas mobility measures are *ideal* for our study since they involve birth cohorts from the late 1970s and early 1980s which are the first treated cohorts in our study.[5]

To measure economic mobility we use the Opportunity Atlas measure of the causal impact of a birth county on a male child's rank in the national household income distribution at age 26, restricting to men whose parents are low income.[6] This measure attempts to estimate the degree to which being born into a given county affects the adulthood earnings of men born into low income households. (See Chetty et al. (2016) for details about how the causal measure is identified and calculated.) To measure civic engagement we use the county-level census response rate, a standard measure of social capital and civic engagment.[7]

4 Estimation

4.1 Estimation Strategy

Our difference-in-difference estimator is a standard two-way fixed specification:

$$y_{ct} = \alpha_c + \sigma_t + \beta HS_c \times After911_t + \epsilon_{ct}, \tag{1}$$

[5]The birth cohorts in question are 1978-1983. See Chetty et al. (2014, 2016) for details.

[6]Low income is defined as parents being in the 25th percentile of their earnings distribution.

[7]Author calculations show this measure is highly correlated with other measures such as the volume or social organizations and informal institutions.

where y_{ct} is the outcome variable for individuals born into county c in year t, α_c and σ_t are birth county and birth year fixed effects, HS_c is an indicator variable equal to 1 if birth county c is a "High Service" county, and $After911_t$ is an indicator variable that equals one if birth cohorts born in year t were 24 or younger at the time of 911. Standard errors are clustered at the birth-county level. We are primarily interested in β, which estimates the impact of being born into a "High Service" county such that you were 24 or younger at the time of 911, conditional on birth-county and birth-year fixed effects. Intuitively, this estimation strategy leverages the quasi-random timing of the 911 attacks interacted with pre-existing variation in civic engagement and economic opportunity measures at the birth-county level.

We also estimate dynamic specifications, in which we measure the impact of being born into a "high service" county a certain number of years before or after the first birth cohort born such that they are age 17 or less at the time of the 911 attacks:

$$y_{ct} = \alpha_c + \sigma_t + \sum_{\tau=-7}^{14+} \beta_\tau 1(t = T + \tau; HS_c = 1) + \epsilon_{ct} \tag{2}$$

.

where the variables are defined as in Equation 1 and T is the birth year 1976, the year before the first birth year in which individuals were 24 or younger at the time of 911. We are primarily interested in the coefficients on the indicators, $1(t = T + \tau; HS_c = 1)$, each of which indicates how many years cohort t in "high service" county c is removed from the first cohort born in county c so as to be 24 or younger at the time of the 911 attacks.

4.2 Identification Concerns

The key identifying assumption underlying Equation 1 is that being born into a high service county relative to the timing of 911 is, conditional on birth county and birth year fixed

effects, unrelated to labor market and family formation measures. For example, if those born into high service counties in key years relative to 911 also experienced shocks other than 911 which affected labor market outcomes, then we could be mistakenly attributing the effects of these other shocks to the military enlistment response exhibited by high service county cohorts in response to 911.

Ex ante, the timing of the 911 attacks interacted with an individual's place of birth seems to constitute quasi-random variation, suitable for identifying causal effects. Any candidate for a confounding labor market shock would *both* have to share the timing of 911 relative to key birth cohorts *and* be targeted towards the several hundred high service birth counties where enlistment rates spiked. The ex ante reasonableness of the identifying assumption derives from the intuition that a confounding shock that cuts across just these birth-cohorts and birth-counties is unlikely.

However, to be cautious, we also explore the question of endogeneity empirically. The main evidence in favor of our identifying assumption is the lack of pre-trend differences in the first stage estimates (Figure 1), and the very large and pronounced enlistment change observed (Figure 2). (We discuss the magnitude of the first stage estimate in Section 5.1.) In particular, Figure 1 shows that prior to 911 "high service" birth counties shared trends with other counties, but experienced lower enlistment shares. After 911, the high service counties see a large (64%) and sustained increase in enlistment, overtaking the control counties in levels of enlistment shares.[8] We are not aware of a candidate shock large enough and targeted in such a way as to give rise to a relative enlistment response this large in magnitude.

Furthermore, as we discuss in Section 5.2 below, we also explore the geographic distribution of high service counties. [Results awaiting Census Bureau disclosure.]

Finally, we note that because the timing of treatment variation does not vary across

[8]Appendix Figure A1 shows the pre-trends for even earlier birth cohorts back to 1960. Trends remain parallel with the exception of the birth cohorts 1968, where the high service county enlistment rate increased relative to control counties.

the sample - *all* cohorts were subject to the same 2001 shock - our two-way fixed effects estimator does *not* suffer from the methodological worries associated with heterogeneous treatment effects (Goodman-Bacon, 2021), Chaisemartin (2018)) and contamination bias (Sun and Abraham, 2020). Below we explore the possible interpretation that, within a birth-county, the military enlistment of older birth cohorts affects that of younger birth cohorts. In particular, we take seriously the idea that older cohorts who enlisted in response to 911 influenced their younger birth-county peers, possibly contributing to the persistence of the first stage estimates and perhaps influencing marriage and fertility outcomes (Section 6.2).

5 Main Results

5.1 Impact of 911 in High Service Birth Counties on Enlistment

5.1.1 Size and Robustness of Enlistment Response

As Figure 2 shows and Table 1 reports, the impact on military enlistment of being born into a "high service" county so as to be 24 or younger at the time of the 911 attacks is *very* large, suggesting an increase in enlistment of 3.4 percentage points (64% of the pre-period mean and 80% of the sample mean). Column 2 of Appendix Table A1 shows that this result is robust to specifications of Equation 1 which include not only birth-county fixed effects but also birth-county specific linear trends. The robustness of the estimates to birth-county linear trends suggests that this increase in enlistment is *not* driven by unobservable differences in birth county trends, but rather by differential responses to the 911 attacks.

5.1.2 Strong Persistence of Enlistment Response

A striking feature of Figure 2 is that the first stage impact is *strongly* persistent. Cohorts born into high service counties well after those aged 18-24 at the time of 911 *still* display

higher rates of enlistment than their counterparts.[9] Therefore, these first stage results suggest that, for cohorts born into "high service" counties, 911 constituted a pronounced and lasting encouragement to civil service in the form of military enlistment. Possible explanations for this persistence feature cross cohort peer-effects. For example, older sibling enlistment might encourage younger sibling enlistment or, more broadly, older classmate or friend enlistment might encourage the enlistment of younger cohorts, even those cohorts who were children at the time of the 911 attacks (Johnson and Lidow, 2016).

5.2 Geographic Distribution of High Service Counties

[These results are awaiting Census bureau disclosure.]

5.3 Heterogeneity of Enlistment Response

From the literature on civic engagement quite broadly and military enlistment more specifically, we expect civic and enlistment responses to differ by gender and race (Han, 2018). Based on the overall demographics of military service members, we expect the enlistment response to be driven by men and White men in particular.[10] Our large sample, which exceeds 13 million observations, enables a high degree of precision even for subpopulations, offering improvements upon existing studies.

Table 2 displays estimates from specifications of Equation 1 which interact the difference-in-difference estimator with sex and race indicator variables. Column 1 of Table 2 repeats the pooled estimate from Table 1. The top row of Table 2 shows that the main estimate of the enlistment response remains unmoved as we interact the difference-in-difference estimator with sex and race variables.

[9] Figure A2 shows that the impact persists through at least 17 birth cohorts.

[10] Cesur et al. (2013) Table A1 displays summary statistics for military enlistment for cohorts similar to those in this study, and reports a sample that is approximately 70 percent white and 80 percent male.

Columns 2-4 of Table 2 reveal that there is substantial variation in the enlistment response by sex and race. Male enlistment nearly doubled (99% increase), while female enlistment declined. White, Non-Hispanic enlistment increased 82%, Hispanic increased 77% and Black increased 66%.[11] Race and sex interactions show that White, Non-Hispanic men and Hispanic men saw the largest increases (145% and 153% respectively) while Black men saw increases of 94%, just below the increase for men as a whole (99%). We conclude that the enlistment response is driven by men and White men in particular.

5.4 Reduced Form Estimates

Estimating Equation 1 with various labor market and household formation measures as the dependent variable, we identify the reduced form impact of being born into a high service county so as to be 24 or younger at the time of 911. Given the literature on the effects of military service (Angrist, 1990, 1993, 1998; Angrist and Johnson, 2000), we expect to find estimates which suggest large labor market gains. We find estimates consistent with, and slightly larger than, expected from the literature. Moreover, given the literature on the relationship between military deployment and family formation variables, we expect military service to increase marital stability for White men (Heerwig and Conley, 2013), but military deployment to increase divorces, especially for deployed female service members (Negrusa et al., 2014; Angrist and Johnson, 2000). Moreover, because our enlistment response estimates are large in magnitude (64% of the pre-period mean) and our sample size is above 13 million, we have the power to identify effects on many key labor market and household formation variables.

[11] Each percentage impact is computed by summing or subtracting the appropriate rows from Table 2 and scaling by the demographic means reported at the bottom of the table. For example, the male enlistment estimate is calculated by summing .031, .058, and .003 from Column (2) and scaling by the mean for men (.093)

5.4.1 Labor Market, Household Formation and Mortality

Table 3 reports reduced form estimates for key labor market outcomes. We find that being born into a high service county so as to be 24 or younger at the time of 911 causes a 1.2 percentage point (4.6%) increase in high school completion, and a .3 percentage point (6.7%) reduction in unemployment with no accompanying increases in the likelihood of being a discouraged worker. This suggests that the newly enlisted enjoy lower unemployment rates without leaving the labor market. Along with these increases in labor market participation, we find increases in wage income of $3,800 per year (9.3%) and increases of .7 working hours per week (1.7%).

The dynamic estimates reported in Figure 3, show estimates from specifications of Equation 2 with different labor market variables as the dependent variable. Each of the four labor market variables display stable pre-trends for birth cohorts older than 24 at the time of 911. For the cohorts 18-24 at the time of 911, we see reductions in high school dropouts (panel a) and unemployment (panel c), and increases in log wage income (panel b) and wage income (panel d). The wage increases are largest for those less than 18 at the time of 911, and may grow slightly across birth cohorts. Unemployment impacts are also largest for the youngest cohorts. By contrast, the increase in high school completion is felt most by birth cohorts through 1988, while the youngest cohorts in high service counties display little difference in high school completion from other counties. Given the persistence of the enlistment response across younger cohorts which is likely attributable to older cohorts influencing younger cohorts (Section 5.1.2), we may expect some of these dynamics, in which the labor market success of older cohorts benefits younger cohorts.

Table 5 display estimates of specifications of Equation 1 in which various household formation measures are the dependent variable. The results suggest that being born into a high service county so as to be 24 or younger at the time of 911, causes individuals to be 3.1 percentage points (4.7%) more likely to marry, and 1 percentage point (8.4%) more

likely to divorce. This suggests that approximately one third of the newly formed marriages end in divorce. We also find that women are 1.3 percentage points (26%) more likely to have children in the past year. We note that since the enlistment response was driven by men (Section 5.3), this fertility increase can be understood *only* as a birth-county cohort level effect and *not* as an effect that pertains only to service members; in other words, the newly identified fertility is *not* from newly enlisted female service members, but from entire birth-county cohorts that experienced both male enlistment responses and female fertility responses to 911.

Alongside the labor market and family formation benefits we identify several costs (Table 7). The newly enlisted were .1 percentage points (14%) more likely to be deceased and .02 percentage points (25%) more likely to be hospitalized. These estimates point to the physical risk borne by active-duty service members, and also to post-service risks of suicide and other harm such as substance abuse (Suitt, 2021). In Section 6.4 below we weight the labor market benefits against these costs and find that for standard discount rates and values of a statistical life the benefits exceed the costs.

5.4.2 Reduced Form Estimate Size

While large in magnitude, the labor market impacts of being born into a high service county relative to the 911 attacks are comparable to labor market impacts from well-known sources such as early childhood programs, compulsory schooling laws and job training programs.

For example, identifying off Head Start rollouts, Bailey et al. (2020) shows that attending Head Start increases the likelihood of finishing high school by 2.4 percentage points (2.7%) (Table 1, Col 6), increases labor income by 4 percentage points (36%) adds 2.3 more work weeks per year (5.6%), and 3 more work hours per week (8.7%) (Table 4, Col 6). While the effects are similar in magnitude, in Bailey et al. (2020) the effects are driven by gains for women, while in our case the enlistment increases are driven by men.

Identifying off compulsory schooling laws in Canada, Oreopoulos (2006) finds that attending an extra grade decreases unemployment by nearly 1 percentage point (Table 6 Col 4), and increases earnings by 7 percentage points (Table 3, Col 3). Identifying off compulsory schooling laws in the United States, Acemoglu and Angrist (2000) finds that an extra year of schooling causes a 7.6 percentage point increase in wages (Table 6, Col 4). Thus, the total state-cohort effect of a compulsory schooling law is similar to, though perhaps slightly lower then, the effect of being born into a high service county relative to the 911 attacks.

Using NLSY97 data, Gaulke (2021) shows that off job training programs increase employment likelihood by 3.7 pp (Table 1), and earnings by approximately $5,000 (Table 3). Using administrative data from Virginia, Meyer et al. (2020) shows that training programs that stack credentials increase employment likelihood by 4 pp and annual wages by $2,300 (7%). Thus, the overall impact of 911 on high service county labor market outcomes is comparable in magnitude to that of job training programs.

6 Robustness: Estimate-Size, Mechanisms, Cost-Benefit

In this section, we consider whether the reduced form estimates discussed in Section 5.4 should be considered to be impacts experienced *only* by the enlisted population ("veterans only") or considered to be impacts pertain to *entire* birth-county cohorts. This discussion begins with mortality estimates for reasons discussed below (Section 6.1). Next, we discuss three potential mechanisms (Sections 6.3.1- 6.3.3). Finally, we conduct a cost benefit analysis that weights mortality risks against labor market gains (Section 6.4).

6.1 Estimate size: Mortality Magnitude Comparison

We start with mortality magnitude comparisons for two reasons.

1. We measure mortality using administrative Social Security records, which are able to

record both active-duty and non-active duty military deaths with a high degree of accuracy.

2. Active-duty military serve in a high-risk environment, and are at a higher risk of suicide than civilians (including after their service ends).

For these reasons, a close examination of the mortality estimates will both provide a highly accurate test of the magnitude of our estimates (because of 1), and allow us to assess whether it is reasonable to interpret the reduced form estimates as effects directly on those who enlist ("veterans only") (because of 2).

Reduced form estimates of the impact on death suggest an effect size of .17 percentage points (14%), which is consistent with summary tabulations of death rates of service members as compared with non-service members.[12] Scaling the reduced form death impacts by the first stage estimates (Table 1), suggests that about 5% of the newly enlisted become deceased in the sample period, which is nearly identical to what standard tabulations (Marrone, 2020; Suitt, 2021) suggest is the share of new military recruits from the post-911 era who are deceased. We note that, unlike many publicly available military death estimates, since our measure is obtained from administrative Social Security records, our mortality measure includes both active-duty and veteran deaths.[13]

Thus, the scaled mortality estimate both corroborates the magnitude of the enlistment response (which we use to scale the reduced form mortality estimate), and also suggests that it may be reasonable to interpret other reduced form estimates as occurring largely

[12]Table 5 Column 1 shows that being born a high service county at a service age increases the likelihood of death by .17 percentage points. Public data sources suggest that approximately .09% of active-duty military die in service. (The latter number is obtained by dividing deaths per year by the total count of active duty military in a year, for years 2001-2016). Other sources of mortality such as suicide are larger for veterans than non-veterans, so the difference in difference estimate is consistent with existing tabulations.

[13]The 5% estimate is obtained by scaling the reduced form death estimate by the first stage estimate, $\frac{.0017}{.03}$. Standard tabulations are taken from Marrone (2020) and Suitt (2021), and computed by finding the annual average number of new recruits in the post-911 era (Marrone, 2020) and using this to scale an estimate of the annual average number of post-911 service member deaths (Suitt, 2021). The Suitt (2021) estimates of deaths includes active-duty deaths as well as non-active-duty deaths such as suicide.

through effects directly on those who enlist ("veterans only"). We explore the limits of this interpretation in the following section.

6.2 Estimate size: Veterans Only?

To further explore the size of the estimates, in this subsection we assume that reduced form impacts are measuring effects experienced *only* by those who newly enlisted ("veterans only"), and not through peer effects present throughout *entire* birth-county cohorts. For example, in the case of the reduced form estimate for unemployment, the veterans only estimate would be accurate if being born into a "high service" county relative to 911 affected county cohort unemployment *only* by changing the unemployment status of the newly enlisted and *not* by affecting the rest of the birth-county cohort.[14] While the mortality estimates suggest this "veterans only" exercise is reasonable, nevertheless, because 1 in 6 households contain a service member (as discussed in Section 1 above) it is likely that many of these reduced form effects are best understood as effects distributed throughout birth-county cohort groups. This is especially important to consider when interpreting family formation variables, since marriage within a birth-county cohort is common,[15] and marriages between veterans and non-veterans are very common.[16]

Scaling the estimates in Table 6 and 3 by the first stage estimates in Table 1 suggests that the newly enlisted were 35% more likely to finish high school and 9% less likely to

[14]This scaling exercise is obviously similar in spirit to standard treatment on the treated estimates, in which an intent to treat estimate is scaled by the first-stage estimate of policy uptake. However, we avoid the terminology "treatment on the treated" since there is not explicit policy variation in our design that is targeted at any population, and there is no sense in which the 911 attacks were targeted at a military response from certain counties. In other words, our reduced form labor market impact estimates hold for entire birth-county cohorts, all of whom are assigned treatment (and not the mere intention to treat) based on birth year and birth-county.

[15]Author calculations from public 2010 ACS 5-yr data show that 67% of married individuals marry someone from their birth state and 57% of veterans marry someone from their birth state.

[16]Author calculations from public 2010 ACS 5-yr data suggest that of households which include married veterans, 86% of these households contained veterans who were married to non-veterans (including marriages that ended in divorce and those who married multiple times)

be unemployed. Moreover, scaling suggests that the newly enlisted earn more than double the income of their counterparts, working approximately 50% more hours.[17] This "veterans only" estimate represents an increase in income from approximately $32,000 to $88,000. (The reduced form estimate discussed in Section 5.4 above represented an increase from $32,000 to $36,000.) Scaling the migration estimates in Table 4 suggest that nearly all the newly enlisted moved out of their birth state,[18] which is consistent with the explanation that migration in search of labor market opportunities is a crucial channel for these labor market impacts (See Section 6.3.1 below for further discussion).

Because marriage within a birth-county cohort is common, family formation variables are less obviously susceptible to a "veterans only" interpretation, but the scaling exercise suggests that the newly enlisted marry at nearly double the rate of their counterparts, with only one third of these new marriages ending in divorce.[19] If approximately half of these newly married household have children, then this is consistent with the estimate in Table 7, which suggests that newly enlisted women are 40% more likely to have children than their counterparts.

Overall the "veterans only" estimates are much larger in magnitude than the reduced form estimates, and larger than well-known estimates of early childhood programs, compulsory schooling and job training programs (Section 5.4.2 above). We interpret the scaled estimate size as a caution against taking the "veterans only" estimate as the main estimate of this study; the mortality estimates are the possible exception to this caveat for reasons detailed in Section 6.1 above.

[17] Obtained by dividing each estimate in Table 3 by .034. For income estimates we interpret the log income estimate as a percentage measure, scale by the first stage estimate and multiplying by average wage income ($\frac{.0928}{.034} \times \$29,000$). Working hour estimates are scaled by the first stage, which suggests a 21 hour increase, which is approximately 50% of the pre-period mean hours (40.94).

[18] Scaling the migration impact by the enlistment impact ($\frac{.0306}{.034}$) suggests that over 90% of the newly enlisted migrated out of their birth state

[19] $\frac{.031}{.034}$ suggest a 91% increase in the marriage rate. $\frac{.011}{.031}$= (divorce estimate)/(marriage estimate) suggests that one third of the newly formed marriage end in divorce.

6.3 Mechanisms

Frequently studied mechanisms for labor market effects of military enlistment include migration, educational attainment, and household formation. Consistent with the related research (Section 2.1), much of the prior research on these potential mechanisms examines the effects of conscription, the effects of enlistment prior to 911, and the effects of deployment only, rather than the effects military enlistment post-911.

Because military service is distinct from many other labor market shocks (such as early childhood program rollouts, state compulsory school laws and federal job training programs) in that migration plays a crucial role for many military personnel, ex ante we expect migration to be a key mechanism, and find strong evidence in its favor (Section 6.3.1). We also consider family formation and educational attainment as mechanisms, but find less evidence for those channels. (Section 6.3.2)

6.3.1 Migration as a Mechanism

Migration estimates in Table 4 reveal that being born in a high service county so as to be 24 or younger at the time of 911 increased migration away from an individual's birth county by 3 percentage points (4%). This increase was *entirely* driven by out of birth state migration (Column 2) rather than within birth state migration (Column 3). Figure 4 confirms these results by showing the dynamics of between state migration as a response to 911.

Overall, these estimates are consistent with the view that between state migration is a significant channel through which enlistment increased labor market outcomes. In fact, out of state migration in particular can explain nearly 90% of the enlistment response[20] Thus, our results are consistent with the account that military enlistment increased migration away from birth counties with lower economic opportunity and towards states and counties with

[20]Scaling the migration impact by the enlistment impact ($\frac{.0306}{.034}$) suggests that over 90% of the newly enlisted migrated out of their birth state

higher economic opportunity.

6.3.2 Household Formation as a Mechanism

There is evidence that marriage and fertility are associated with labor market gains, and may also be causal factors in the determination of wages.

There is a large literature identifying the marriage wage premium using cross-sectional data. (See Antonovics and Town (2004) for an overview). Antonovics and Town (2004) shows that identifying the marriage wage premium restricting to monozygotic twins still yields premium estimates above 20%. Similarly, de Linde Leonard and Stanley (2015) conduct a meta-analysis of the literature and find a marriage wage premium just above 10%. However, using NLSY79 data and focusing on the timing of male wage movements, Killewald and Lundberg (2017) finds that the most likely account of the apparent marriage premium is that men marry as their wages are already rising and divorce when they are already falling. Our estimates are consistent with an account in which marriage is the unique channel through which high service county birth increased wages.[21] However, because marriage within a birth-county cohort is common,[22] and there is some doubt about whether marriage is causally responsible for wage increases, it is unlikely that enlistment responses are increasing wage through the *unique* channel of a marriage premium.

Using NLSY79 data, Cowan and Kamarck (2014) documents a 6% father wage premium. This premium could explain up to 67% of the labor market impacts we find. However, using administrative Danish data from 1980-2013, Kleven et al. (2019) shows that women's earnings, but not men's earnings, decrease following the birth of a first child, and that the resulting wage gap persists in the long-run. Given that the enlistment response we observe

[21] If the 3 percentage point increase in military enlistment were driven by the same 3 percentage point increase in the marriage rate, then a marriage wage premium of approximately 10% could explain all of the wage increases we observe.

[22] Author calculations from public 2010 ACS show that 67% of married individuals marry someone from their birth state and 57% of veterans marry someone from their birth state. See Section 6.2

is driven by men (Section 5.3), it is therefore unlikely that labor market impacts could be caused *entirely* by increased fertility.

6.3.3 Education as a Mechanism

One possible mechanism through which the 911 attacks could have increased wages in high service counties, is through increasing educational attainment alongside increasing military enlistment. For example, for earlier born cohorts, it is possible that the desire to enlist served as motivation to finish high school. It is also possible that in high service counties enlistees benefited from new found veteran educational benefits such as the GI Bill that made college less expensive by easing credit constraints (Barr, 2015).

We do not find strong evidence in favor of the view that educational attainment is the main mechanism for the labor market gains we observe. Standard estimates of the impact of high school completion on labor market outcomes, would explain at most 20% of estimated labor market effects. Moreover, our estimates for college completion do *not* suggest a convincing causal impact in high service counties; if anything the results suggest that being born into a high service county so as to be 24 or younger at the time of 911 may have *slightly* decreased bachelor degree attainment. The details are as follows:

The literature on the impact of high school completion on wages give a consistent account of labor market benefits. Identifying off compulsory schooling laws in Canada, Oreopoulos (2006) finds that attending an extra grade decreases unemployment by nearly 1 percentage point (Table 6 Col 4), and increases earnings by 7 percentage points (Table 3, Col 3). Identifying off compulsory schooling laws in the United States, Acemoglu and Angrist (2000) finds that an extra year of schooling causes a 7.6 percentage point increase in wages (Table 6, Col 4). (See also Goldin and Katz (2011) for a broader discussion.) However, Stephens Jr and Yang (2014) shows that introducing regionally specific year of birth effects renders many of these estimates statistically insignificant.

Nevertheless, taking estimates from Acemoglu and Angrist (2000), we could explain the impact of being born into a high service county on wage income only by assuming that the educational increases we find (a 1.2 percentage point increase in high school completion) constitute increases of 1.3 years of school. [23] Therefore, if the 911 attacks constituted a civil service shock that gave rise to increased high school completion and military enlistment, that shock would have to increase educational attainment by more than a year for the educational increases to explain the wage gains, which we find unlikely since even compulsory schooling laws increased educational attainment by a quarter of a year or less (Acemoglu and Angrist, 2000), Table 4, Col 1, row 3). Thus, while we certainly cannot rule out that high school completion is an important mechanism, we conclude from these standard estimates that it is likely to explain less than 20% of our wage estimates.[24]

There is also a large literature on the role that military enlistment plays in college completion, much of which focuses on the post-911 GI bill (Barr, 2015; Barr et al., 2021). We do not find convincing evidence that the enlistment response to 911 impacted college completion in high service counties; if anything, it *slightly* decreased college completion (Figure A3). This does *not*, of course, imply that military enlistment decreased college going. Because high service counties are also counties with low economic mobility (especially for men born into low income households), we might expect college going in these counties to be lower than in other birth counties. The lack of a clear finding on college completion could reflect higher college going due to enlistment being canceled out by the otherwise lower college attainment we would predict for these low mobility birth-county cohorts. Overall, while our results are consistent with the view that military enlistment increased college completion and thereby wages, we find no new evidence in favor of this mechanism.

[23] Calculated as 7.3 pp wage increase per year of schooling $\times$ 1.3 years = 9.3 pp wage increase

[24] Calculated as 7.3 pp wage increase per year schooling $\times$ .25 additional years = 1.8 pp increase on average. 1.8 is 19% percent of 9.3.

6.4 Quantifying the Welfare Impacts

To consider the welfare impacts of the 911 shock to enlistment, we compare the labor market benefits of being born into a high service county so as to be 24 or younger at the time of 911, to the mortality costs. The intuition behind this comparison is that the newly enlisted enjoy a stream of higher earnings, but also face an increased probability of death, and we wish to compute the magnitude of the wage benefits in relation to the added risk of death.

Greenberg et al. (2021) find that the average value of a statistical life (based on observed U.S. soldier mortality rates 2002-2010) is generally between $500,000 and $900,000. Table 8 shows the ratio of the wage benefits to the morality risks as a function of the value of a statistical life and the intertemporal discount rate. For example, if the value of a statistical life is $500,000 and the discount rate is 20%, we find that the wage benefits are 38 times the cost of the mortality risks. And, if the value of a statistical life is $1 million these wage benefits are still 19 times the cost of the mortality risks. These cost to benefit calculations do not take into the costs of increased hospitalization (Table 7), which would attenuate the benefit to cost ratio towards zero. However, Greenberg et al. (2021) also note that, among men in combat occupations, the estimated value of a statistical life does not reach $1.67 million until the mortality rate reaches the 97th percentile of their mortality distribution. Table 8 shows that even at $5 million with a 20% discount rate, the labor market benefit to cost ratio is about 4.

7 Conclusion

We investigate the effect of military service on labor market, health and family formation outcomes, leveraging differential changes in enlistment rates brought about by the September 11^{th} attacks (911). Using restricted microdata, we identify hundreds of "high service" counties in which certain birth-county cohorts exhibit sharp and persistent enlistments responses

to 911. We find that individuals born into "high service" counties between 1977 and 1983 (who were aged 18-24 at the time of the attack), enlisted at nearly twice the rate of earlier birth cohorts (who were older than 24 at the time of the attack). This increased enlistment rates persists for at least a decade of birth cohorts after 911. These "high service" birth-county cohorts experienced a 10% increase in wages, decreased unemployment and impacts on other labor market measures. These birth-county cohorts also experienced increases in key household formation measures such as marriage and fertility. When we turn to health measures, we find that these birth-county cohorts also experience increases in hospitalization and mortality rates. Weighing the labor market benefits against the mortality costs, we show that, within standard ranges for the value of a statistical life and the intertemporal discount rate, the labor market benefits outweigh mortality costs.

The existing literature on the effects of military service has investigated pre-911 era enlistment, often relying on conscription rules, propensity score matching, or instrumental variables for identification (Angrist, 1990; Angrist and Krueger, 1994). Overall, the literature presents mixed and sometimes inconclusive evidence on labor market outcomes. Cesur et al. (2016) and Cesur et al. (2018) use post-911 era outcomes to study the effects of combat exposure (rather than the effects of enlistment more broadly) on mental health and criminality. Our work fills a gap in the literature by producing causal estimates of post-911 enlistment on labor market, family formation, and health outcomes. The effects of post-911 enlistment are directly relevant to emerging domestic and foreign policy considerations, as the War in Afghanistan ends.

Finally, our estimates could be interpreted to show a surprising situation in which birth-county cohorts born into areas with low economic mobility, attained relative labor market success because those counties featured higher levels of civic engagement. This environment of civic engagement may have increased military enlistment in the face of an unexpected call to duty, the 911 attacks. For certain birth cohorts, military enlistment may have been the

channel through which civic engagement paradoxically "overcame" otherwise low economic mobility.

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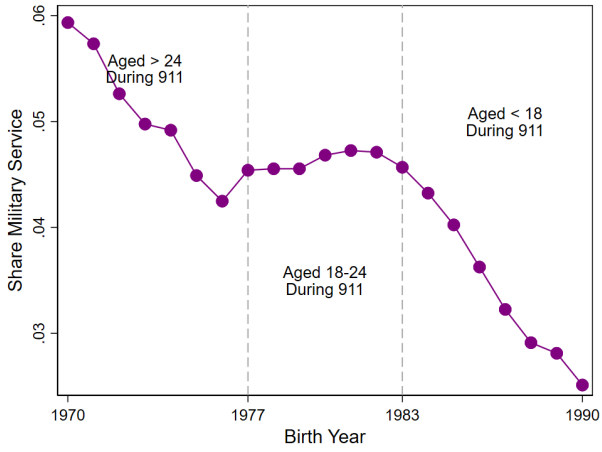
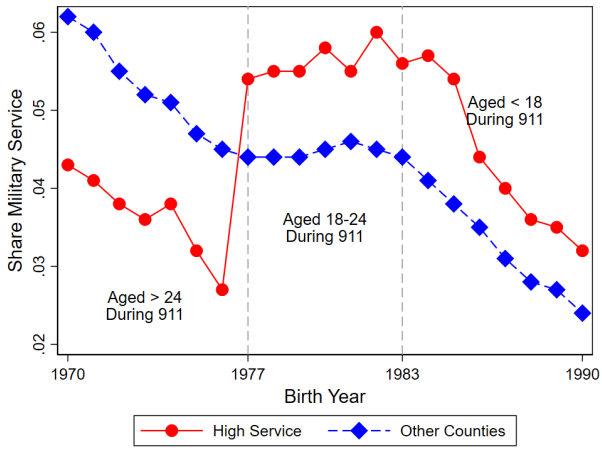
Figures

Figure 1: Trends in Enlistment



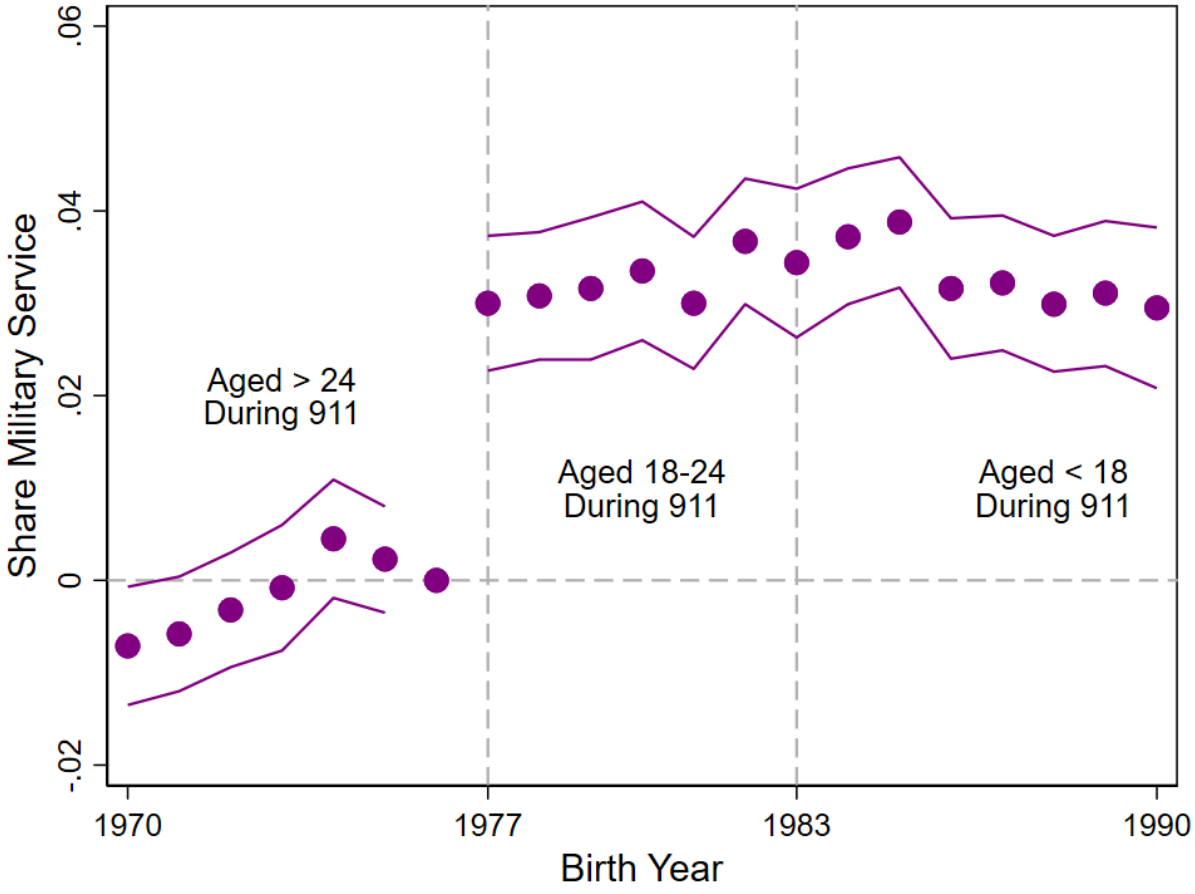
(a) Relative to 911



(b) Relative to 911 by High Service County

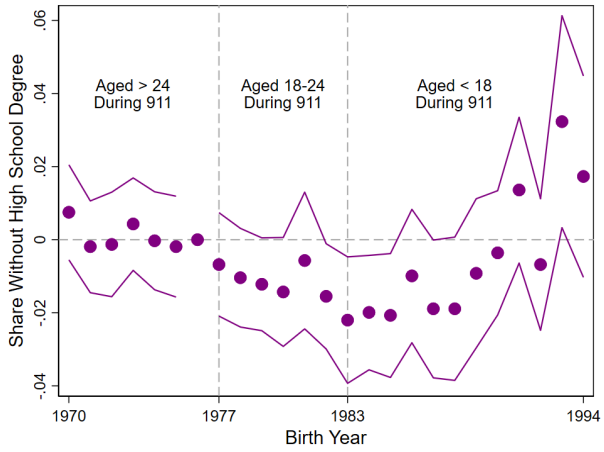
Note: Figure shows trends in enlistment rates by birth cohort relative to the timing of the 911 attacks. Panel (b) further shows breakdowns in enlistment rates by 'high service" birth county status. High service counties are defined and discussed in sections 2.3 and 5.2 above. Source is the 2005-2018 ACS linked to Social Security records.

Figure 2: Impact of Being Born into High Service County and 911

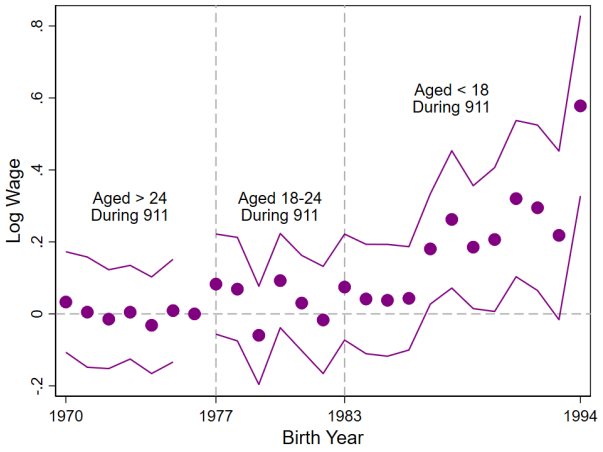


Note: Figure shows estimates and 95% confidence intervals from specifications of Equation 2 in which the outcome variable is military enlistment. Birth cohort 1976 is the reference year. Estimates include birth county and birth year fixed effects. Source is the 2005-2018 ACS linked to Social Security records.

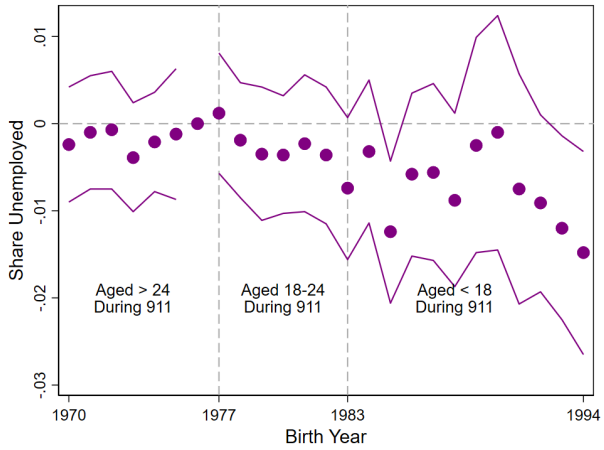
Figure 3: Labor Market Impacts



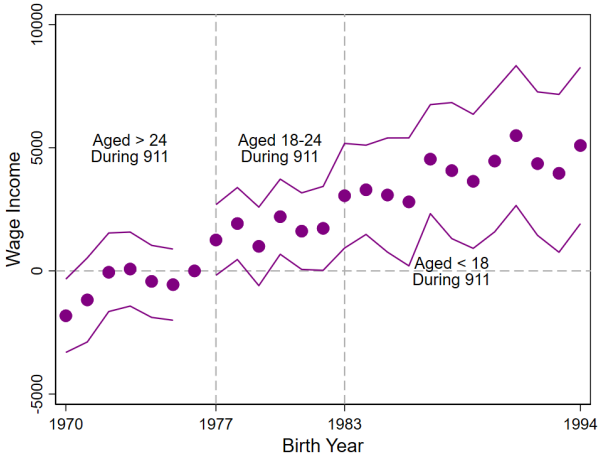
(a) Less than High School or HS



(b) Log Wage Income



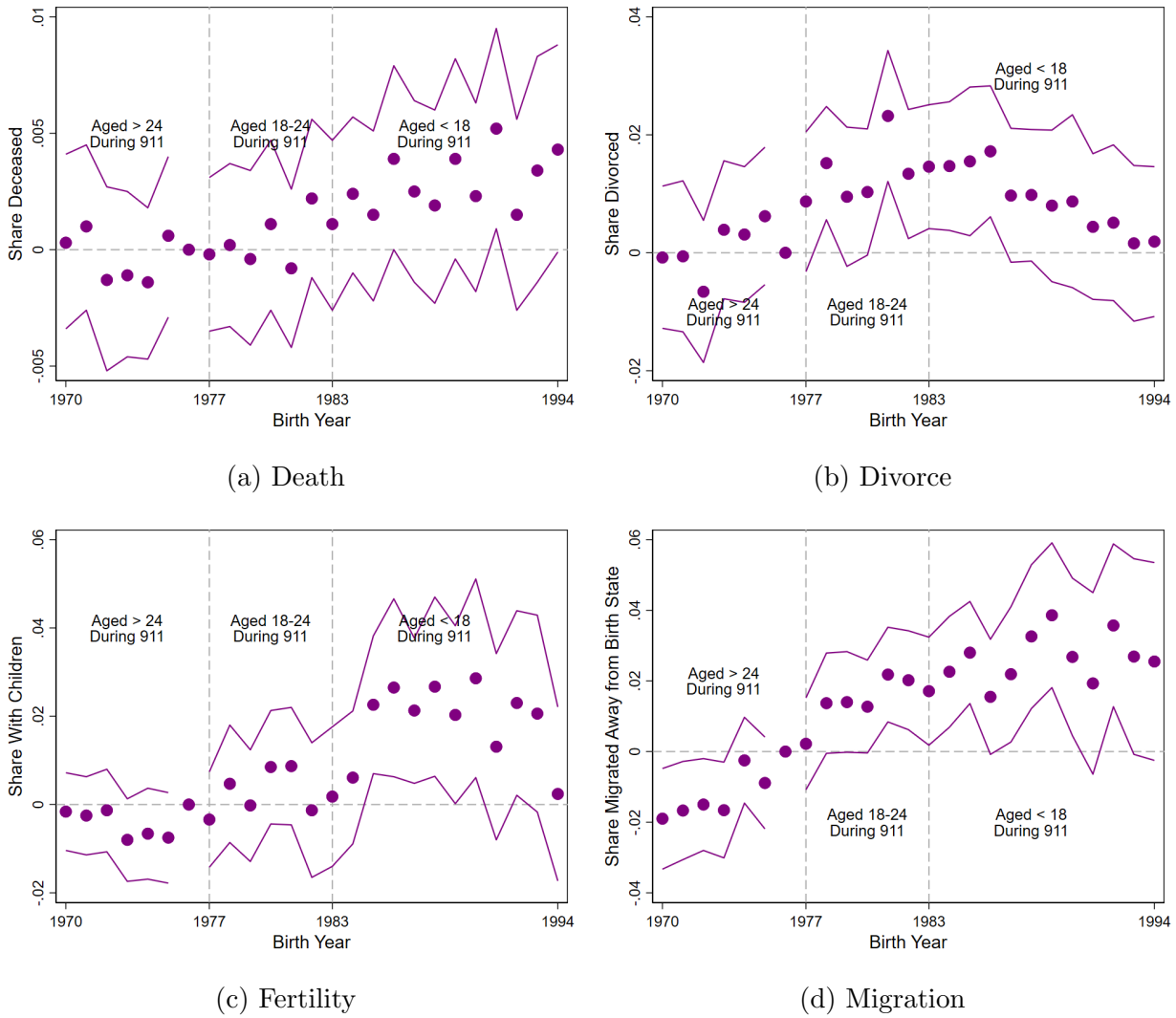
(c) Unemployed



(d) Wage Income

Note: Figure shows estimates and 95% confidence intervals from specifications of Equation 2 in which the outcome variable differs in each panel. Birth cohort 1976 is the reference year. Estimates include birth county and birth year fixed effects. Source is the 2005-2018 ACS linked to Social Security records.

Figure 4: Health, Family Formation and Migration



(a) Death

(b) Divorce

(c) Fertility

(d) Migration

Note: Figure shows estimates and 95% confidence intervals from specifications of Equation 2 in which the outcome variable is military enlistment. Birth cohort 1976 is the reference year. Estimates include birth county and birth year fixed effects. Source is the 2005-2018 ACS linked to Social Security records.

Tables

Table 1: Impact of Being Born In High Service County and 911

	(1)
Military Enlistment	
High Service X After 911	0.0340***
	(0.0011)
Observations	13890000
Mean	0.0429
Mean (Pre-Period)	0.0535

Note: Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$).

Table 2: Impact of Being Born In High Service County and 911: Breakdowns

	(1)	(2)	(3)	(4)
Military Enlistment				
High Service X After 911	0.0340***	0.0315***	0.0358***	0.0304***
	(0.0011)	(0.0013)	(0.0011)	(0.0015)
High Service X After 911 X Male		0.0033		0.0088***
		(0.0029)		(0.0024)
High Service X After 911 X Black			-0.0028	-0.0008
			(0.0036)	(0.0038)
High Service X After 911 X Hispanic			-0.0059**	-0.0025
			(0.0026)	(0.0025)
High Service X After 911 X Black-Male				-0.0045
				(0.0055)
High Service X After 911 X Hispanic-Male				-0.0064
				(0.0051)
Male		0.0583***		0.0627***
		(0.0008)		(0.0007)
Black			0.0011	0.0104***
			(0.0006)	(0.0005)
Hispanic			-0.0037**	0.0037***
			(0.0014)	(0.0011)
Black-Male				-0.0175***
				(0.0007)
Hispanic-Male				-0.0141***
				(0.0020)
Observations	13890000	13890000	13090000	13090000
Mean (Pre-Period)	0.0535			
Mean (Male)		0.0927		
Mean (White)			0.0575	
Mean (Black)			0.0653	
Mean (Hispanic)			0.0362	
Mean (White Male)				0.1
Mean (Black Male)				0.1068
Mean (Hispanic Male)				0.0639

Note: Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$). Demographic variables in Columns (2)-(4) are indicator variables for respondent being male, being Black or being Hispanic.

Table 3: Impact of Being Born In High Service County and 911: Labor Market

	(1)	(2)	(3)	(4)	(5)	(6)
Labor	Unemployed	Discouraged	Zero Wages	Hrs Worked	Log Total Income	Log Wage Inc
High Service X After 911	-.003*	-0.00014	-0.0013	.7268***	.0943***	.0928***
	(.0015)	(.0026)	(.0031)	(.0876)	(.0279)	(.0345)
Observations	13890000	13890000	13890000	11450000	13890000	13890000
Mean	0.0656	0.2095	0.2061	38.01	8.658	7.874
Mean (Pre-Period)	0.0451	0.1688	0.1977	40.94	9.352	8.368

Note: Each column reports estimates from a separate regression. Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$).

Table 4: Impact of Being Born In High Service County and 911: Migration

	(1)	(2)	(3)
	Any	Between State	Within State
Migration			
High Service X After 911	0.0307***	0.0306***	0.0001
	(0.0049)	(0.0049)	(0.0004)
Observations	13890000	13890000	13890000
Mean	0.6686	0.6618	0.0068
Mean (Pre-Period)	0.7065	0.6996	0.0069

Note: Each column reports estimates from a separate regression. Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$).

Table 5: Impact of Being Born In High Service County and 911: Household Formation

	(1)	(2)	(3)	(4)	(5)
	Currently Married	Divorced	Never Married	Times Married	Fertility
High Service X After 911	0.0310***	0.0115***	-0.0422***	-0.0044	0.0136***
	(0.0062)	(0.0026)	(0.0048)	(0.0062)	(0.0041)
Observations	13890000	13890000	13890000	5920000	6959000
Mean	0.4186	0.0795	0.4991	1.153	0.0766
Mean (Pre-Period)	0.6586	0.1376	0.1983	1.212	0.0528

Note: Each column reports estimates from a separate regression. Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$).

Table 6: Impact of Being Born In High Service County and 911: Education

	(1)
	Less Than High School
High Service X After 911	-0.0120***
	(0.0037)
Observations	13890000
Mean	0.2572
Mean (Pre-Period)	0.267

Note: Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$).

Table 7: Impact of Being Born In High Service County and 911: Mortality and Hospitalization

	(1)	(2)
	Deceased	Hospitalized
High Service X After 911	0.0017**	0.0002*
	(0.0008)	(0.0001)
Observations	13890000	13890000
Mean	0.0091	0.0006
Mean (Pre-Period)	0.0121	0.0008

Note: Each column reports estimates from a separate regression. Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$).

Table 8: Cost Benefit Analysis of Military Service

	Value of a Statistical Life (Millions of $)						
	0.5	1	5	10	15	20	50
Discount Rate							
0	228.0	114.0	22.8	11.4	7.6	5.7	2.3
0.05	118.5	60.5	12.1	6.1	4.0	3.0	1.2
0.1	72.0	36.6	7.3	3.7	2.4	1.8	0.7
0.15	50.0	25.2	5.0	2.5	1.7	1.3	0.5
0.2	37.9	19.0	3.8	1.9	1.3	0.9	0.4

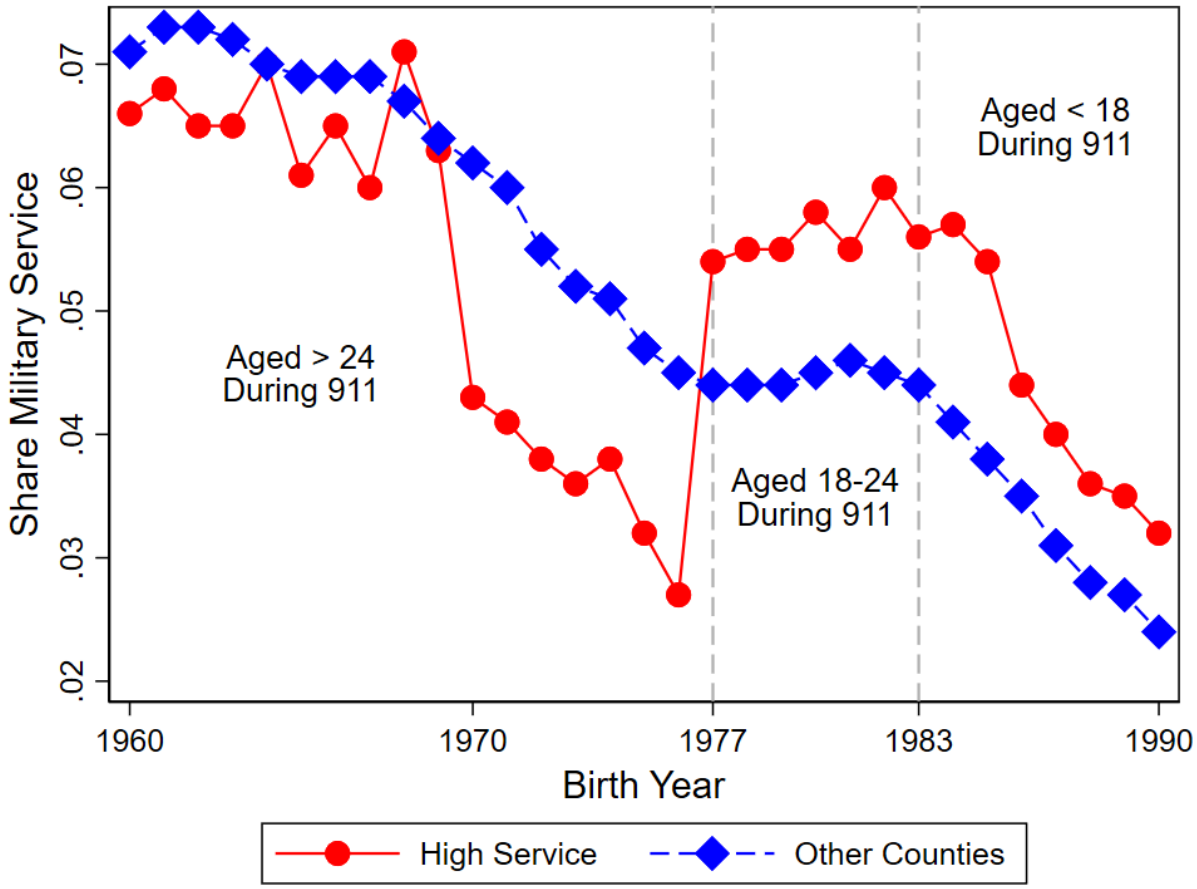
Note: Each cell gives the ratio of lifetime wage benefits of military service to the mortality costs of military service, drawing on estimates above in Tables 1 and 5. Wage benefits are calculated as the 30 year sum, present value discounted according to the equation:

$$PV = \$3,800 \times \frac{1-d^{31}}{1-d}; d = \frac{1}{(1+r)} \tag{3}$$

where r is the discount rate given on the left-hand side column. Costs are calculated by taking the value of a statistical life in a given column and multiplying by the increased mortality risk from Table 5 above.

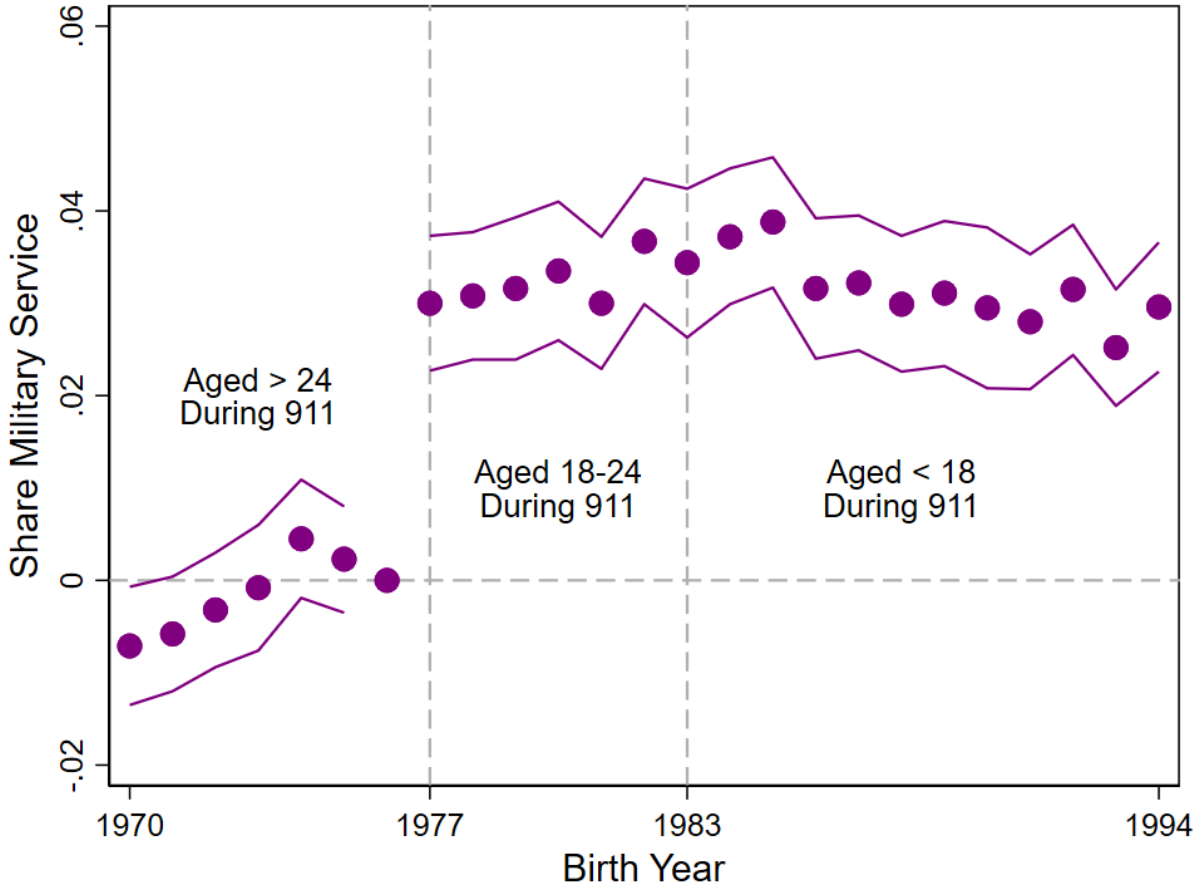
Appendix Figures

Figure A1: Trends in Military Service by High Service Birth County and Birth Year: Extended Pre-Period



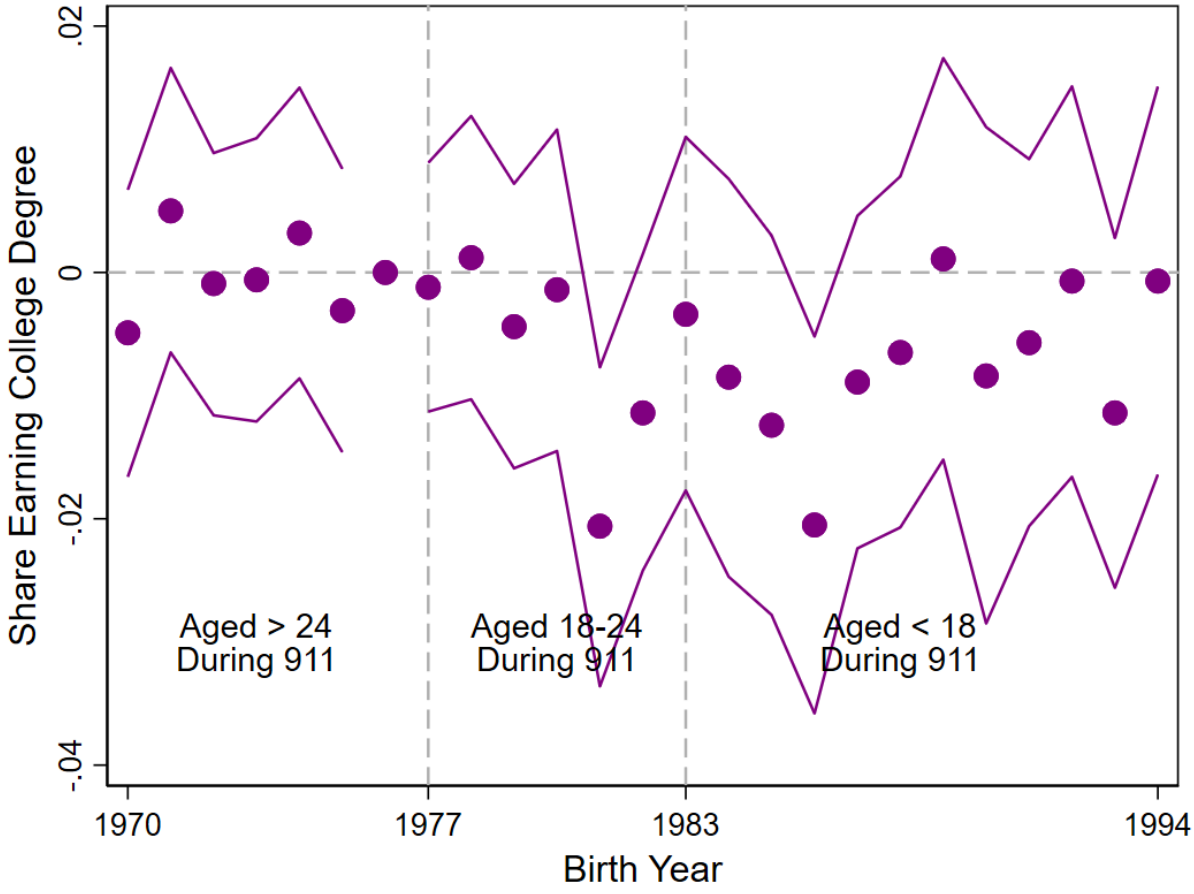
Note: Figure follows panel (b) of Figure 1 in showing breakdowns in enlistment rates by 'high service" birth county status. High service counties are defined and discussed in sections 2.3 and 5.2 above. The source is the 2005-2018 ACS linked to Social Security records. This Figure extends Figure 1 by showing the pre-period birth cohorts back to 1960.

Figure A2: Impact of Being Born into High Service County and 911: Extended Post-Period



Note: Figure shows estimates of Equation 2 in which the outcome variable is military enlistment. Birth cohort 1976 is the reference year. Estimates include birth county and birth year fixed effects. The source is the 2005-2018 ACS linked to Social Security records. This Figure extends the estimates in Figure 2, showing estimates through 1994 birth cohorts.

Figure A3: Educational Impact



Note: Figure shows estimates and 95% confidence intervals from a specification of Equation 2 in which the outcome variable is the share of those earning a college degree. Birth cohort 1976 is the reference year. Estimates include birth county and birth year fixed effects. Source is the 2005-2018 ACS linked to Social Security records.

Appendix Tables

Table A1: Impact of Being Born In High Service County and 911: Linear Trends

	(1)	(2)
Military Enlistment		
High Service X After 911	0.0340***	0.0341***
	(0.0011)	(0.0028)
Observations	13890000	13890000
Mean	0.0429	0.0429
Mean (Pre-Period)	0.0535	0.0535
County X Year Trends		X

Note: Standard errors are clustered at birth county level and reported parentheses. Specification is Equation 1, and includes birth-county and birth-cohort fixed effects. The sample is restricted to 1970-1994 birth cohorts. Source is the 2005-2018 ACS linked to social security records. Significance levels indicated by: * ($p < .10$), ** ($p < .05$), ***($p < .01$). Column (1) of this Table repeats the estimates from Table 1, while Column (2) reports estimates from specifications of Equation 2 which includes birth-county linear trends.